

Being Precise about Precision and One-to-one Specificity

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Abstract: Following from my criticism of Calcott's analysis on the permissive/instructive distinction, I rebut his claims that 1) he clarifies my measure of one-to-one specificity; 2) for all intents and purposes of his analysis his notion of precision is different from my measure of one-to-one specificity; 3) Waddington box is a better and different model than the extension of Woodward's radio I propose.

Main Text

In Bourrat (2019a), I showed two things. First, that Calcott (2017) did not link his analysis to Woodward's (2010) notion of one-to-one specificity. Calcott argued instead that his notion of precision is not captured by Woodward's analysis on specificity. This claim came to a surprise to me as I clearly saw a link between precision and one-to-one specificity, one of Woodward's two dimensions on causal specificity. This led me to confront Calcott's claim that the notion of specificity cannot be used to discriminate some cases in which a cause has a precise effect, from other situations in which a cause can have a range of effects. Based on one of Calcott's own example, I showed, *contra* his claim, that if by 'specificity' one means 'one-to-one specificity' these cases do come apart when an adequate information-theoretic measure known as variation of information is used. In light of this, I then proposed that the permissive/instructive distinction can be explained away by considering the two dimensions of specificity proposed by Woodward, that is, the degree of fine control a cause exerts on its effect, or INF—measured by mutual-causal information—and the extent to which to one causal value corresponds exactly one causal effect, or one-to-one specificity—measured by variation of causal information.¹

In his response to my criticism Calcott (2019) argues three things: First, that my measure of one-to-one specificity “goes awry”; Second, that contrary to what I claim the two dimensions of causal specificity proposed by Woodward do not permit to capture the permissive/instructive distinction; Third, that extending Woodward's radio to show that it is equivalent to Waddington box is inadequate.

¹ As a side note, Calcott claims that I have used not two but three measures of specificity (2019, p. 4). I never proposed a third measure but rather claimed multiple times that there are two dimensions for the notion of causal specificity and that, to have a fuller picture of a given situation, it is interesting to measure specificity on both dimensions. I take this to be different from proposing a third measure.

With respect to the first point, Calcott spends more than a third of his response to show that by subtracting my measure of one-to-one specificity to 1, one can link it to mutual causal information and that it allows us to talk about ‘specificity’ rather than ‘(un)specificity’, a term I used to link my measure to one-to-one specificity. This is quite disconcerting. I myself drew, although in a different way, the direct link between the two measures as it is explicitly stated in equation (1) and (2) in Bourrat (2019a) and in Bourrat (2019b) which Calcott cites. The relationship between mutual information and variation of information is stated anywhere the definition of variation of information is presented. Calcott, however presents his ‘alternative’ measure as if I did not draw this link, omitting any mutual information term when he defines variation of information while they were in my definitions.

There are numerous measures related to mutual information one can come up with. The real question is whether anything is clarified by Calcott’s alternative measure. The answer is a resounding no, unless stating that a glass is half empty clarifies the statement that the same glass is half full. Thus, although I gladly accept that Calcott’s proposition is more elegant than mine, in no way does his analysis clarify, correct or represents an alternative to mine.

With respect to the second point, I will not repeat my analysis here but make instead a few remarks clarifying why one-to-one specificity amounts to precision in Calcott’s examples. But first, let me note that Calcott proposes that a permissive cause may be seen as a ‘canalised switch’ (2017, p.495). I certainly agree that *some* permissive causes can be regarded as such and that Calcott’s analysis might be targeted at those type of causes, but this does not represent the consensus on what a permissive cause is. This should be obvious from the different quotes I provided in Bourrat (2019a, Table 1). The biologists I cite, for the most part, use the distinction in the way I do, where ‘permissive’ can be equated with ‘background condition.’ This is perhaps a hint that the permissive/instructive distinction has more than one meaning and is thus after all more elusive than Calcott seems to believe. This remark aside, I clearly stated in my criticism that my main target was Calcott’s claim that the notion of precision is not captured by Woodward analysis, when in fact it is. One crucial point to note is that Calcott’s notion of precision is at the core of his analysis on the permissive instructive distinction. Showing that his notion of precision is captured by specificity surely would show that Calcott’s analysis is partly mistaken. Importantly, Calcott claims that his measure of precision and my measure of specificity are related but different. If by ‘different’ Calcott means that his measure is a component of mine but that this difference plays no role in the examples he uses, then we agree, but once again this is stretching what the word ‘different’ means. There is indeed a straightforward way to show why for all intents and purposes in Calcott’s (2017) the two measures are equivalent.

In using his Waddington box, Calcott (2017, p. 493-495) contrasts two setting p_3 and p_4 and claims that a measure of specificity cannot account for their difference. The outcome is indeterministic (or fuzzy) in p_3 while it is deterministic (or precise) in p_4 . Calcott (2017, fn. 7) claims what is distinct about p_4 is that the conditional entropy of the effect on the cause is nil or more formally that $H(E|\hat{C}) = 0$. I recall here that one condition for maximal one-to-one specificity I propose in Bourrat (2019a, 2019b), using variation of causal information, is $VI(E;\hat{C}) = H(E|\hat{C}) + H(\hat{C}|E) = 0$. Thus, Calcott’s measure is a component of mine.

Now, in his examples p_3 and p_4 , Calcott assumes that the number of states for the cause (his S variable) is lower than the number of states for the effect (his B variable) *and* that one state of the effect cannot be produced by more than one state of the cause. This necessarily implies that

$H(\hat{C}|E) = 0$. Thus, for the purpose of comparing the *only* situations proposed by Calcott—which are supposed to present biological situations—our conditions are perfectly equivalent. However, in some biologically realistic situations, such as when the same effect can be produced by two distinct causes which produce no other effects, his measure of precision would only tell that each cause has a precise effect. Based on this criterion alone, it would be indistinguishable from p_4 , while it would not with the measure I proposed, which is the only reason why I claimed that it is more general.

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To sum up, Calcott (2017) claimed to have found an alternative to specificity, when it was just one component of Woodward’s one-to-one specificity. In his response to my criticism, he very timidly recognizes that he did not see the link between the two in his original paper and presents links between different measures which I acknowledged and are well known. Crucially however, what Calcott claimed was not possible with specificity and Woodward’s analysis, namely discriminating p_3 from p_4 —one important exemplar which motivated Calcott to present his analysis—is perfectly possible when one refers to one-to-one specificity. To me, this shows that, at least in part, his analysis went astray.

Finally, Calcott claims that extending Woodward’s radio provides no benefits to the analysis, worse it is inadequate. In Bourrat (2019a), I showed that by *merely* adding one more dial which would make the radio indeterministic—one does not hear the same thing every time the second dial is in one given position and the other variable are the same—the radio becomes equivalent to Waddington box. Calcott claims that by adding a second dial we do not have a radio anymore and that the metaphor is lost. This is supposed to persuade us that Waddington box is more relevant. Yes, we do not have a radio anymore, but it connects to the intuitive example of the radio. We *almost* have a radio, and this is the crucial point. Citing more than 3 million possible layouts for sure looks impressive but one could argue that it adds unnecessary complications and asks the reader to switch example and learn new variables. All this for which benefit? A Waddington box which refers to nothing in the world but itself. In the end, the same analysis can be conducted with a second dial on the radio. Is a Waddington box really closer to a real biological example than a radio with a second dial? I remain sceptical. Two slots, a ball, four buckets, and different layouts of pins are intuitively not much closer to real biology than a radio with two dials and one switch. Waddington box’s complexity—or more accurately intricacy—seems furthermore to go against Calcott’s own stated aim which is “to construct a simple model which generates, as best we can, the same intuitions as the biological cases.” (2019, p. 8)

While I rebutted Calcott’s main affirmations against my analysis, showing that they detract from the main points I made, I would like to finish on a more positive note. That following Woodward’s analysis permits us to discriminate situations which Calcott claimed one could not does not mean that Calcott’s analysis has no value. Clearly, examining what occurs when controlling for a variable—which is analogous to conditioning on a probability distribution—is an important aspect of experimental sciences. It permits us to learn more details about the interactions between different variables than if one were to look at things more globally, that is without conditioning.

² Calcott confuses a one-to-one *relationship* with a *measure* of one-to-one specificity when he claims: “Bourrat goes on to claim that his measure is “more general” than my own, but this cannot be correct. If something is one-to-one, then it is precise, but not vice-versa. This makes the one-to-one relation more restrictive, rather than more general.” I obviously agree that a one-to-one relationship is more restrictive than one which can be many-to-one. This however tells us nothing about the measure of one-to-one specificity. Many-to-many relationships score a certain value on one-to-one specificity. By “more general”, I meant that my measure can deal with a larger range of biological situations.

My main target, however, was Calcott's claim that precision captures something which is not captured in Woodward's analysis.

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